



EGUsphere, referee comment RC1
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Comment on egusphere-2022-209

Anonymous Referee #1

Referee comment on "Relationship between the stocks of carbon in non-cultivated trees and soils in a West-African forest-savanna transition zone" by Tegawende Léa Jeanne Ilboudo et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-209-RC1>, 2022

General comments:

In light of the significant role played by tropical landscapes in sequestering atmospheric carbon (C), understanding the dynamics of above- and below-ground C stocks is essential to developing sustainable land use management in such regions. This paper was aimed at examining the relationship between stocks of carbon in non-cultivated trees and their surrounding soils in different land uses in a West African forest-savanna transition landscape. In spite of the fact that this aim is somewhat apparent in the paper's title (except the land use), it is extremely troubling that the paper fails to frame and thus address any specific problem regarding this relationship and to set forth a hypothesis for testing. The impact of land use (different configurations) on soil organic C stocks and their relationship has been adequately studied, so it is important for readers to know the gaps and how they can be resolved. As a result, I fear this paper lacks focus and is incoherent in content. This may be due, in part, to the weak definition of non-cultivated trees and its conceptual relation to land use. I find it challenging that such a class of trees are located even in forests and bushlands. Were the forests plantations? What is the area of influence of these non-cultivated trees in cultivated and non-cultivated fields? Were the soils sampled within this area of influence accordingly? Also, it should be noted that the study area (100 km²) is neither large enough nor its findings extrapolated to cover the Forest-Savanna transition in West African landscapes, despite the suggestion inherent in the title. There seems to be an unjustified attempt throughout the paper (e.g. the captions of the table and figures) to generalize its empirical findings to the entire West African region. This, along with the title of the manuscript, is misleading and inaccurate. Along with these flaws, the paper is also poorly written. In order to assess this manuscript for its rigor, it must be thoroughly edited for consistency in grammar and clarity. Even after reading the manuscript multiple times, it was still challenging evaluating the merit of the methodology and the conclusions and discussion, which I found shallow and poorly presented. Due to these factors, it is difficult to argue that this paper provides any new knowledge or makes any contribution to the current body of knowledge, especially on this fascinating topic of terrestrial C dynamics. In my opinion, this paper must be rewritten and resubmitted before it can be considered. I specify some concerns below for authors to consider when revising the paper.

- Abstract:

- There is no explicit statement on the objectives/aims/questions of the paper, and hence how they were achieved/answered. Also, be consistent on the study area as this is an empirical work that does not cover the whole forest-transition zone of West Africa.

- Introduction:

- There is complete lack of context and too many generalizations without a critical assessment of the current state of literature. I suggest to rewrite the introduction providing clear context on the research problem by engaging the contemporary literature on terrestrial carbon dynamics. This can help readers to appreciate the exact contribution of this work.
- Against the claims of the paper, there is no testable hypothesis provided. In line 74-75, the hypothesis of the paper is given as "...we hypothesized that the relationship between the stock of soil organic C and the stock of C in the uncultivated will not be identical in all land uses", which is a statement of fact and not a hypothesis. It is indeed a historical fact that land use influence above and below ground C stocks. Also, this hypothesis is unfalsifiable.

- Materials and methods:

- I suppose the lack of context and a testable hypothesis in the introduction also affected this methods section. This is because, while it is clear that the sampling design of this work follow the LSDF design, it is largely unexplained as to how the aims of this work align with those of the LSDF, given that the original design was to provide baseline information for land degradation processes. Also, what are some of the unique features of the selected area that makes it useful and representative for the aims/objectives of the study. Which of these features are generalizable and which are not?
- In line 95, "The LSDF as it uses a nested hierarchical sampling design allows for the development of predictive models that has a global coverage without changing the local relevance", please explain.
- It is inadequate to state that "land use classification was done using a simplified version of the FAO Land cover classification system", please explain how and why it was implemented in this study? As it is, I fail to understand why a global classification system is used for such a small local study.

- Line 98-99, in which way is the contribution to SOC of annual crops different from that of perennial crops?
- Line 99-100, please explain how the data on the impacts of erosion, fire and grazing were collected? Also, be specific on the topographic features that were collected and how they were collected.
- What informed the reason for counting only trees that had a diameter of >2.5 cm and a height of >1.5? And why was the radius for tree data collection different in the annual croplands compared to the others?
- In section 2.3, line 122-124, was the pH of the soils measured? Because it is inadequate to assume that the values for some soils in other parts of West Africa will automatically apply to your own soils.
- Line 125-126, which specific packages in R (it is important for developers of such packages to be acknowledged whenever possible)
- Please add the information on the calibration plots (from the spectroscopy data) to the main paper, and not in the supplementary.
- How many soil samples were collected in total, and how many constituted the 15%? How representative was this 15% regarding the feature space?
- Line 125-132, this whole paragraph needs to be re-written to improve clarity.
- Line 140-144, please clarify and explain the basis for using bulk density values from Burkina Faso when the soils and the ecological conditions are so different. One would assume values from other neighboring countries with similar agroecological conditions might be applicable.
- It is largely unclear to me why the path analysis was used especially as there is no hypothesis to be tested. Obviously, the path analysis is a statistical analysis and so it should be part of the same section

- Results and discussion

- Sections 3.1 and 3.2 are quite basic information that should have been provided in the methods section or seem to be irrelevant to the work. Figure S1 is a table, not a figure. There is no table 1 in line 175.
- As to be expected the main finding was that SOC stocks vary with land use... of course they do. In Line 263-268, the argument to support the reason why perennial crops had on average higher SOC is weak and contradictory, please check.
- Bizzare conclusion: " Our results suggest that perennial crops cultivation and bushlands preservation in the forest – savanna transition zone of northeast Tiéningboué should be recommended for soil organic carbon preservation". It is quite difficult to draw such a conclusion from a single empirical study, please revise.